# LETTER TO THE EDITOR

This is a reply to those who have taken issue with ideas expressed in my paper, "Convergence in American Orioles." The criticisms of my arrangement and the counter proposals by G. G. Williams (*Wilson Bulletin*, 63: 52–54) are based on the importance he assigns to the plumage of female and young birds. He claims it is "axiomatic" that this be taken prominently into consideration in making any phylogenetic arrangement.

The facts do not appear to support Williams' assumption that this plumage represents the true genetic picture of the species. Sex-inversion experiments involving gonadectomy or hormone injections show that in some groups of birds sexual dimorphism may be genetic while in others it is the resultant of pituitary gonadotropins or gonadal hormones or a complex interaction between them (see Danforth in "Sex and Internal Secretions," E. Allen, editor). In orioles the year-round male plumage and total absence of a female plumage in most species suggests that the former is genetic and the latter under hormonal control. While the female plumage could be an expression of gene physiology, the increasing incidence of sexual dimorphism northward in México in races of *Icterus pustulatus* (van Rossem, 1938. *Bull. Brit. Ornith. Club*, 58: 138) strongly suggests environmental control via the pituitary. Moreover, where plumage dimorphism is lacking, females have the plumage of males, not the reverse.

Actually, ornithologists are cautious in using female and juvenal plumage to imply relationship because it is too often absent to serve as a reliable index—and this is especially true in the orioles. I do think the female plumage (plain olive or streaked) in dimorphic species tends to be a throw-back to types, possible primitive, which are still common among Old World insect-eaters. But this conspires to bring out, in bright-hued species of finches, warblers, tanagers, weaver-birds or even starlings (*Aplonis*, *Cinnyricinclus*), female plumages that are similar to those in female orioles or blackbirds (*Agelaius*). And this reversion to a common, inconspicuous type can hardly be considered an index of close relationship comparable with the distinctive plumage patterns of males. Moreover, in oriole species in which both adults have male plumage the juvenals often are protectively colored and it seems logical that the latter may need this protection even when the more experienced female does not. In fact protection appears to be the reason for plumage dimorphism and female plumage is very plastic, changing markedly within the populations of a single species, as van Rossem pointed out. Williams' conclusion that this "female" plumage indicates a yellow ancestry for all orioles is not "inescapable." It is escapable.

Incidentally, Williams also mis-states my case. I do not say that *all* orioles come from a black ancestor. I distinctly say that in my opinion *Icterus* arises from a yellow ancestor (*Xanthopsar*), and I draw a picture of it (Fig. 6).

If Williams' initial assumption concerning the phylogenetic importance of female and juvenal plumage is false, his counterproposals for an arrangement of orioles based on it are likewise false. I am hardly justified in taking space to refute them point by point, since any reader of my paper will know my answers in most cases. Taking his main thesis, we might ask where in Central America is the yellow blackbird from which to derive yellow orioles? Or, if that seems unfair—what is the genetic mechanism by which *Icterus gularis*, *I. pustulatus*, and *I. pectoralis* combine their segregated bill and palate characters in a single, variable descendent species, *I. nigrogularis*, in northern South America? In my paper I contend that these three arise from *nigrogularis*, segregating its variations. It does not work backwards. I am unable to believe in a Central American origin of orioles which then spread into South America. When we try to derive species from species, range by range, the Williams hypothesis breaks down repeatedly. Any ornithologist studying a group will make a number of such schemes for testing, but an hypothesis has no reality—does not exist outside the mind—until it accounts for all of the facts.

I find Williams' proposal somewhat frightening. Space did not permit me to go more deeply into taxonomy and my desire to explore an interesting biological phenomenon (the convergence) tempted conjecture, but I hope my paper will not be regarded by younger ornithologists as a signal for unsupported hypotheses. I consider mine rather well supported.

The complete ignoring, in Williams' superficial hypothesis, of the ecological picture is inexcusable since this picture is known (and given in my paper). In view of the demonstrated plasticity of female plumage I see no justification for putting Cayenne Orioles of dark forest thickets in a different phyletic group from, say, Hooded Orioles of the desert simply because the former lack plumage dimorphism. This is too obviously an environmental effect: one might as well attempt to separate *Agelaius humeralis* of Cuba from all mainland *Agelaius* (from which it is derived) for the same reason. Williams' static morphological arrangement is that of a taxonomist of 100 years ago. It reflects no knowledge of behavior, ecology or geology. Based entirely on color resemblance, it takes no account of evolution under environmental pressures. That type of ornithology is still the best we can do for some parts of the world, but is hardly permissible for the American orioles in the 1950's.

Fr. Haverschmidt's note on *I. nigrogularis* (*Wilson Bulletin*, 63: 45) shows ecological tolerance for the species not previously reported. He finds it confined to swampy areas in Surinam while Todd and Carriker (1922. "The Birds of the Santa Marta Region of Colombia," p. 472) say it is "abundant in the semi-arid coast belt" and quote Simons to the effect that it prefers the cacti and acacias of the hot valleys to the cool forests.

For Bond's opinions (*Wilson Bulletin*, 62: 216) regarding the distribution of West Indian birds I have the greatest respect, but I am unable to see how the absence of *Agelaius* and *Bananivorus* from Jamaica is accounted for by the presence of *Nesopsar nigerrimus* and *Icterus leucopteryx*, even if extinction of the one is implied from the presence of the other. Direct evolution on Jamaica of one oriole from the other is 'out' because they belong to distinct phyletic lines, and this is also true for the blackbirds. That arboreal *Agelaius humeralis* was forced from the Cuban marshes by *A. phoeniceus assimilis* is also incredible because the latter is a very slightly modified recent arrival from the mainland and *A. humeralis* must have required a very long time to achieve its arboreal adaptations.

Since Williams' and Bond's objections both arise partly out of an unwillingness to recognize an additional oriole genus I feel obliged to re-affirm my belief in one. Due to the convergence, my diagnosis must be unconventional—based as it is on the fact that, wherever the two genera are found together, *anywhere in their ranges*, *Bananivorus* is decidedly smaller. But it is also true that the chestnut found in the plumage of the Cayenne Oriole persists in some small degree in all other species of *Bananivorus* except *cucullatus*, *auricapillus* and *parisorum*, the forms most convergent with forms of *Icterus*. But these three make unmistakable, compact *Bananivorus* nests, sewing *through* the leaf. Moreover, the very fact that the orioles of these two genera have distinctly different distributions on the islands of the Caribbean, suggests that they were distributed at different times.

Ornithologists who know its evils have every right to fear the splitting of genera. My own tendency is in the opposite direction but the orioles are numerous in species and the splitting is in the best interest of a classification expressive of phylogeny. A classification that cannot recognize cases of convergence fails to express evolution and is, therefore, one of convenience only. We might as well classify all yellow birds, or all red birds, together. That, too, would be classification of convenience—but it would not be phylogeny.

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